The papers of most interest to readers of THE REVIEW will probably be those relating to marriage, the family and fertility, although some of the contributions on interactions of demographic and social forces and on migration are also relevant. The following items seem to stand out:

Social Values about Family Size in the United States (R. Freedman).
De la fécondité instinctive à la fécondité élective (Père de Lestapis).
Recent Fertility Trends in England and Wales (B. Benjamin).
Fertility Differentials in Trinidad (G. W. Roberts), and
The Social-Psychological Structure of Fertility (C. F. Westoff).

Freedman argues that in recent years young Americans have reached a consensus that a family of two to four children is ideal; apart from Roman Catholics (higher) and working wives (lower) this ideal is common to all strata. Westoff, giving data derived from the new Princeton inquiry into the future fertility of two-child families, confirms previous findings that finances are the most important single factor affecting the prospects of a third birth. Benjamin provides a pithy summary of the present-day position in Great Britain. Roberts gives some results of a new inquiry in which residents in Trinidad who are not East Indians are shown to experience decreasing fertility as their educational attainment rises.

The contribution of Père de Lestapis is of a somewhat different character. He discounts the idea that "religious practice" is a directly determining factor in human fertility and suggests, very reasonably, that truly spiritual *behaviour*—in the widest sense—is far more significant. Although this is a subject for general, rather than numerical, discussion, the author also suggests a line of statistical research which might help one to investigate the effects, *inter alia*, of a sincere religious attitude to preparation for marriage and to participation in family life. It is an unusual approach of particular interest.

P. R. C.

PHYSIOLOGY

Goodfield, G. J. *The Growth of Scientific Physiology*. London, 1960. Hutchinson. Pp. 174. Price 18s.

PHYSIOLOGY AS A subject grew during the eighteenth and nineteenth centuries by the application of the scientific method to the study of how animals work, and was stimulated, in particular, by the medical importance of the results and the intriguing character of the problems involved. G. J. Goodfield deals with the history of ideas associated with one aspect of this growth, and traces the interaction between traditional attitudes of mind and the rising power of the experimental method. The book is primarily concerned with the history of the problem of the origin and control of animal heat, partly because this was the concern of a succession of the best biological scientists of the period, partly because this theme appropriately illustrates the course of the conflict between vitalism and mechanism. By the end of this period, physiology was in fact established for a short time "as a valid science," as suggested by the blurb on the front cover, although the loosely integrated parts have since radiated along separate paths in the formation of a range of subjects such as biophysics, pharmacology, neurophysiology, psychosomatics and so forth. Physiology in its heyday was the study of the functions and interactions within organisms at the organ level.

The book is an excellent historical account of theories of animal heat during the period from Harvey to Claude Bernard. Written in a clear masterful style, amply illustrated by quotation, and impressive with evidence of familiarity with the subject-matter, it is a lesson in the art of teaching the history of ideas. The presentation, and therefore the understanding, of the methodological problems involved in formulating and testing theories is clear. Perhaps, then, the actual content is less important. Within the science of the study of living organisms, many topics could have been used to illustrate the same theme This thought naturally leads to the question of the intention of the book and the audience to which it is aimed. What is the purpose of a perusal of theories of animal heat in the last two centuries?

In answer, I think that two classes of reader should know of this book: the cultured scientist or medical man interested in the history of his subject, and the student of the history of science. The scope of the book does, however, suggest that a few years of research among books have been written up as a book without much reference to the needs of potential readers.

The "Resolution of the Mechanist-Vitalist Dispute" promised by the title of the final chapter never appears. Goodfield himself advises that "the philosophically-minded historian of science can hardly take sides in a dispute as we have been considering. He will probably decide, instead, to keep one foot firmly planted on each side of the fence." This is indeed the sensible position if he is confined (as Goodfield apparently is) to evidence drawn from scientific papers up to the time of the death of Claude Bernard—1878. But in recent years several aspects of this problem have been carried much farther in ways not hinted at in the book under review. The part played by automatic feed-back self-regulating systems in maintaining bodily functions that were formerly enigmatic now provides explanations which show some of the pointlessness of the mechanist—vitalist altercation. Another section of the same dispute is now displaced by the realization that molecular structures, of which we are all made (for of what else, if not) are dynamically changing chemical systems, so that structure determines process and process determines new structure in endless interrelationships. The rest of the dispute mostly goes overboard with the realization of the limitations set by methods of observation and available techniques. We cannot take hold of a single molecule and ask it how it lives. Such topics are beyond the edge of the manageable universe and therefore only of *academic* interest. A passage on p. 162 reveals the essential weakness of Goodfield's attitude to the man at the bench in a white coat: "though we may confidently suppose that physiologists and biochemists will one day uncover these *mechanisms* in all their complexity, they will have done nothing thereby to *explain away* the special character of the associated functions that are our starting point and pose our problem." Goodfield reveals his kinship by his reference to "our." The man at the bench is trying to find out something quite different and pitches his sights much lower, with more confidence and more chance of success. The effect of the whole book, in part produced by comments such as that quoted, is of a collection of pieces of coloured glass and old pottery turned up among only the deeper layers of old refuse, by a searcher after continuity of ideas.

ADRIAN HORRIDGE

GENETICS

Fuller, J. L. and **Thompson, W. R.** *Behavior Genetics.* New York and London, 1960. Wiley. Pp. x + 396. Price 72s.

THIS BOOK COULD have been called "The genetical control of behaviour". It is a compilation of researches on variation and heredity in behavioural characteristics, chiefly in mammals but not neglecting other groups including insects. After two introductory chapters there is an account in rather abstract terms of the experimental methods employed in this field; special attention is given to the methods peculiar to studies of man. Mathematical techniques are included in the text. Other chapters are on variation in sensory abilities, in motor patterns of behaviour, in intellectual qualities and in "personality"; there is a useful chapter on behavioural (mental) illness.

Data from experiments on laboratory animals are given within chapters mainly about man. No barrier is admitted between physiology and behaviour and pains have been taken to relate the facts of overt behaviour to whatever is known about individual variation in neural and endocrine function.

The comments on general principles are balanced. The authors are not only interested in both behaviour *and* physiology but also ready to take data from zoological studies of a variety of species—not only white rats and rhesus monkeys in psychological laboratories. They do not, however, deal at all adequately with the subject of "instinct", under either that name or any other. Yet for their subject problems concerning the fixity or otherwise of behavioural patterns in development are fundamental. As a corollary, they fail to discuss the development of behaviour effectively. This deficiency is part of a more general one: there are far too many isolated facts or quasi-facts